# Pumping the Pipeline: State Lactation Accommodation Laws and Maternal Employment Retention

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**Abstract**

Between 1995 and 2022, thirty-four U.S. states enacted workplace lactation accommodation laws. I exploit this staggered adoption in a triple-difference design—comparing women aged 25–34 to men and older women (45–54) across treated and untreated states—using Census Quarterly Workforce Indicators. Across separation rates, hiring rates, employment, and earnings, I find precisely estimated null effects at the aggregate level (separation DDD = 0.0016, SE = 0.0013). Placebo tests confirm clean identification. These aggregate nulls do not establish that lactation mandates are ineffective. They are consistent with (a) no effect on mothers, (b) effects too small to detect in data pooling all women 25–34 (only ∼4% are postpartum in a given quarter), or (c) effects concentrated in subgroups masked by aggregation. A 1 percentage point reduction in separation among postpartum mothers would appear as only 0.04 points in the aggregate measure, well within the standard error.

**JEL Codes:** J13, J16, J21, J32, K31

**Keywords:** lactation accommodation, breastfeeding, maternal employment, workplace policy, triple-difference

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## 1. Introduction

Consider a nurse in Tennessee who returns to work six weeks after giving birth. She wants to continue breastfeeding—the American Academy of Pediatrics recommends at least twelve months—but she needs somewhere private to pump every three hours. Before Tennessee's 2006 lactation accommodation law, her only option was a bathroom stall. If the discomfort and indignity became too much, she could quit. Whether laws eliminating this friction actually change that calculus is the question this paper addresses.

The policy landscape for breastfeeding in the workplace has expanded dramatically over the past three decades. Beginning with Texas and Utah in 1995, thirty-four states have enacted laws requiring employers to provide reasonable break time and a private, non-bathroom space for nursing mothers to express breast milk (National Conference of State Legislatures, 2023). The federal government followed in 2010 with the Break Time for Nursing Mothers provision of the Affordable Care Act, which covered hourly workers, and again in 2022 with the PUMP for Nursing Mothers Act, which extended protections to salaried employees. Despite this legislative momentum, the causal effect of these protections on maternal employment outcomes remains entirely unknown.

This gap is striking given the depth of the surrounding literatures. Economists have extensively studied maternity leave policy (Baker and Milligan, 2008; Rossin, 2011; Stearns, 2015; Rossin-Slater, 2018), the child penalty in earnings (Kleven et al., 2019; Angelov et al., 2016), and the broader determinants of female labor force participation (Goldin, 2014; Blau and Kahn, 2017; Bertrand et al., 2010). Public health researchers have documented the association between workplace accommodation and breastfeeding duration (Guendelman et al., 2009; Hawkins et al., 2007; Tsai, 2016). Yet no study has brought causal identification to the question of whether lactation accommodation laws affect the maternal employment margin most directly at stake: whether a new mother stays at her job or leaves.

This paper fills that gap. I exploit the staggered adoption of state lactation accommodation laws across thirty-four states between 1995 and 2022 in a triple-difference framework. The identifying variation comes from three dimensions: sex (female vs. male), age (25–34, the peak childbearing years, vs. 45–54, past childbearing), and treatment status (states that adopted laws vs. states that did not). This triple-difference absorbs state-specific shocks common to all demographic groups, national trends specific to any sex-age cell, and state-by-time shocks unrelated to the sex-age interaction. I use Census Quarterly Workforce Indicators (QWI), which provide administrative-quality employment counts, hires, separations, and earnings for every state-quarter-sex-age cell from 2000 through 2022.

The main finding is a precisely estimated null at the aggregate level. The DDD estimate for the separation rate is 0.0016 (SE = 0.0013), implying that lactation accommodation laws do not reduce separation rates for the broad population of women 25–34 by more than 0.4 percentage points (at the 95 percent confidence level) relative to the counterfactual. The results for hiring rates (0.0013, SE = 0.0013), log employment (0.0005, SE = 0.007), and log earnings ($-0.005$, SE = 0.012) are similarly indistinguishable from zero. These null results survive exclusion of early-adopting states, a parsimonious specification, and a Sun–Abraham event study. Critically, placebo tests confirm that neither men of childbearing age nor women past childbearing age show spurious effects, supporting the identifying assumptions.

This null is economically informative, but requires careful interpretation. The aggregate data cannot distinguish between three possibilities: (a) lactation laws have no effect on any mother's employment decision; (b) effects exist but are too small to detect in a population where postpartum mothers are a small share; or (c) effects are concentrated in subgroups—particular industries, firm sizes, or worker types—that are masked by aggregation. Several mechanisms could explain a genuine null among mothers. First, employers in female-intensive sectors may have already been providing accommodation voluntarily, so the mandate was not binding. Second, the constraint that binds for new mothers may not be pump breaks but rather the broader bundle of scheduling rigidity, commuting costs, and inadequate parental leave. Third, even if the laws matter for mothers specifically, the QWI data aggregate across all industries and firm sizes, potentially masking effects concentrated in sectors where accommodation was previously rare.

This paper contributes to three literatures. First, it provides the first causal evidence on lactation accommodation policy, filling a notable gap between the maternity leave literature (Baker and Milligan, 2008; Rossin-Slater, 2018) and the breastfeeding public health literature (Guendelman et al., 2009). Second, it adds to the growing body of evidence on workplace amenities and the gender gap (Wiswall and Zafar, 2018; Mas and Pallais, 2017; Goldin, 2014), demonstrating that not all accommodation mandates produce measurable labor market effects. Third, it illustrates the value of well-powered null results in policy evaluation: the confidence interval rules out moderate-to-large effects (SDE $> 0.05$), providing useful information for policymakers weighing the costs and benefits of workplace mandates.

The remainder of the paper proceeds as follows. Section 2 describes the institutional landscape of lactation accommodation laws. Section 3 presents the QWI data and sample construction. Section 4 details the empirical strategy. Sections 5 and 6 present the main results and robustness checks. Section 7 discusses implications, and Section 8 concludes.

## 2. Institutional Background

Workplace lactation accommodation laws address a specific friction in the return-to-work decision for nursing mothers. A woman who wishes to continue breastfeeding after returning to work must express milk approximately every two to three hours using a breast pump. This requires ten to twenty minutes of uninterrupted time in a private, clean space—not a bathroom—with access to electricity and a place to store expressed milk. Without employer accommodation, the practical barriers can force an early transition to formula feeding, which carries health costs for both mother and child (American Academy of Pediatrics Section on Breastfeeding, 2012; Victora et al., 2016), or can push the mother out of the labor force entirely.

**State Law Adoption.** The first wave of state laws arrived in the mid-1990s. Texas and Utah both enacted legislation in 1995 encouraging employers to support breastfeeding employees. Minnesota followed in 1998, and Georgia and Hawaii in 1999. A second wave in the early 2000s brought California, Connecticut, Illinois, and Louisiana into the fold (all 2001). The mid-2000s saw a cluster of adoptions: Virginia (2005), Mississippi, Oklahoma, and Tennessee (2006), Montana, New Mexico, New York, and Oregon (2007). A third wave around 2008–2009 added Colorado, Indiana, and Vermont (2008) and Arkansas, Maine, North Dakota, and Washington (2009). Later adopters include Maryland (2013), South Carolina (2014), Kansas (2015), Alaska (2016), Michigan (2018), Nevada and New Jersey (2019), and West Virginia (2021). Table 5 provides the complete chronology.

The laws vary in their stringency, but most share common elements: employers must provide "reasonable break time" for an employee to express breast milk during the workday, and a "private location, other than a bathroom" for this purpose (National Conference of State Legislatures, 2023). Some states go further, requiring employers to provide refrigeration for milk storage (e.g., California, Illinois), prohibiting retaliation against employees who exercise their rights (e.g., New York, Oregon), or specifying that employers with fewer than a certain number of employees are exempt. The variation in law content is a limitation of the present analysis, which treats all laws as equivalent.

**Federal Policy.** At the federal level, the landscape shifted twice. The Affordable Care Act of 2010 included the Break Time for Nursing Mothers provision (§4207), which amended the Fair Labor Standards Act to require employers to provide break time and private space for nursing mothers. Crucially, this federal mandate covered only employees eligible for overtime pay—roughly 80 percent of hourly workers but no salaried employees (U.S. Department of Labor, 2010). This left an estimated nine million salaried workers unprotected. The PUMP for Nursing Mothers Act, signed in December 2022, closed this gap by extending protections to all employees covered by the FLSA, including salaried workers, teachers, nurses, and farmworkers.

For identification purposes, the 2010 federal mandate is absorbed by the state-by-quarter fixed effects in the DDD specification, since it affected all states simultaneously. State laws that predated the federal provision provided additional protections beyond the federal floor (e.g., covering salaried workers, providing stronger enforcement). I end the sample in 2022Q4, before the PUMP Act's effective date, to avoid contamination from the second federal expansion.

## 3. Data

The primary data source is the Census Bureau's Quarterly Workforce Indicators (QWI), a set of economic indicators derived from the Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides quarterly counts of employment, hires, separations, and average earnings at the state level, disaggregated by sex and age group, among other dimensions (Abowd et al., 2009). I use the sex-by-age tabulation with total industry (all NAICS sectors) for all fifty states plus the District of Columbia.

The unit of observation is a state-quarter-sex-age cell. I use two sex categories (male, female) and two age groups: 25–34 (the "childbearing" group, which overlaps with peak fertility ages) and 45–54 (the "control" group, past typical childbearing). The sample spans 2000Q1 through 2022Q4, yielding 92 quarters. After dropping cells with missing or zero employment (typically due to Census disclosure avoidance), the analysis sample contains 18,072 observations across 51 state-equivalents.

**Outcome Variables.** I construct four outcomes. The *separation rate* is quarterly separations divided by beginning-of-quarter employment (Sep/Emp), capturing the intensity of worker exits. The *hire rate* is quarterly hires divided by employment (HirA/Emp), capturing inflows. *Log employment* (log(Emp)) measures the stock of workers. *Log earnings* (log(EarnS)) measures average monthly earnings for workers employed at both the beginning and end of the quarter ("stable" workers). The separation rate is the primary outcome, as the theory predicts that accommodation reduces the friction that drives new mothers to leave their jobs.

### 3.1 Summary Statistics

**Table 1:** Summary Statistics by Sex and Age Group

| | Employment | | Sep. Rate | | Hire Rate | | Earnings ($) | |
|---|---|---|---|---|---|---|---|---|
| Group | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Female 25-34 | 244,887 | 276,400 | 0.2150 | 0.0426 | 0.2145 | 0.0457 | 2,802 | 726 |
| Female 45-54 | 231,391 | 250,442 | 0.1239 | 0.0261 | 0.1218 | 0.0269 | 3,624 | 1,076 |
| Male 25-34 | 269,122 | 306,403 | 0.2289 | 0.0428 | 0.2304 | 0.0461 | 3,806 | 875 |
| Male 45-54 | 254,605 | 281,794 | 0.1307 | 0.0284 | 0.1279 | 0.0286 | 6,149 | 1,655 |

Notes: N = 18,072 state-quarter-sex-age observations across 51 states, 2000–2022. Employment is beginning-of-quarter count. Separation rate = quarterly separations / employment. Hire rate = quarterly hires / employment. Earnings are average monthly earnings for stable workers. Source: Census Quarterly Workforce Indicators (QWI).

Table 1 presents summary statistics by sex and age group. Several patterns are consistent with known labor market regularities. Women aged 25–34 have lower average employment counts and lower earnings than men of the same age, reflecting the well-documented child penalty in this age range (Kleven et al., 2019). Separation and hire rates are higher for the 25–34 age group than for the 45–54 group for both sexes, consistent with greater job mobility earlier in careers. The mean separation rate across all cells is 0.175, with a standard deviation of 0.060.

## 4. Empirical Strategy

### 4.1 Triple-Difference Design

The identification challenge is that states adopting lactation laws may differ systematically from non-adopting states, and that changes in female employment could reflect broader trends unrelated to lactation policy. The triple-difference design addresses both concerns by exploiting within-state, within-time variation across sex and age groups.

The estimating equation is:

$$\begin{aligned} Y_{s,q,g,a} = \beta \cdot (\text{Female}_g \times \text{Young}_a \times \text{Post}_{s,q}) + \gamma_1(\text{Female}_g \times \text{Post}_{s,q}) \\ + \gamma_2(\text{Young}_a \times \text{Post}_{s,q}) + \alpha_{s,g,a} + \delta_{g,a,q} + \phi_{s,q} + \varepsilon_{s,q,g,a} \end{aligned} \quad (1)$$

where $s$ indexes states, $q$ indexes quarters, $g$ indexes sex, and $a$ indexes age group. $\text{Post}_{s,q}$ equals one if state $s$ has adopted a lactation accommodation law by quarter $q$, and zero otherwise. For never-treated states, $\text{Post}_{s,q} = 0$ in all periods.

The parameter of interest is $\beta$, which captures the differential effect of the law on women of childbearing age relative to the triple of counterfactuals: men of the same age, women past childbearing age, and untreated states. The three-way fixed effects $\alpha_{s,g,a}$ absorb all time-invariant differences across state-sex-age cells. The sex-age-quarter fixed effects $\delta_{g,a,q}$ absorb any national trends specific to each demographic group (e.g., secular changes in female labor force participation for 25–34-year-olds). The state-quarter fixed effects $\phi_{s,q}$ absorb all time-varying state-level shocks that are common across sex and age groups (e.g., state-level recessions, concurrent policy changes).

Standard errors are clustered at the state level, the level at which treatment varies.

### 4.2 Identifying Assumptions

The DDD requires that, absent the lactation law, the difference in outcomes between women aged 25–34 and the comparison groups would have evolved similarly in treated and untreated states. Formally, let $Y^0$ denote potential outcomes absent treatment:

$$\mathbb{E}[Y^0_{s,q,\text{F,Young}} - Y^0_{s,q,\text{M,Young}} - Y^0_{s,q,\text{F,Old}} + Y^0_{s,q,\text{M,Old}} | s \in \text{Treated}] = \mathbb{E}[\cdot | s \in \text{Control}] \qquad (2)$$

for all post-treatment quarters $q$. This is weaker than the standard DiD parallel trends assumption because it allows for differential trends between treated and control states that are common across sex-age groups, and for differential trends across sex-age groups that are common across states.

I probe this assumption through two placebo tests. If the identifying variation is valid, lactation laws should have no effect on: (1) men aged 25–34, who are not biologically affected by breastfeeding, and (2) women aged 45–54, who are past typical childbearing age. Null effects on both placebos would support the identifying assumptions.

## 5. Results

### 5.1 Main Results

**Table 2:** Effect of State Lactation Accommodation Laws on Labor Market Outcomes

| | (1) Sep. Rate | (2) Hire Rate | (3) Log Emp. | (4) Log Earnings |
|---|---|---|---|---|
| Female × Young × Post | 0.0016 | 0.0013 | 0.0005 | -0.0046 |
| | (0.0013) | (0.0013) | (0.0070) | (0.0116) |
| Female × Post | -0.0019 | -0.0019 | -0.0167 | -0.0023 |
| | (0.0017) | (0.0017) | (0.0134) | (0.0078) |
| Young × Post | -0.0022 | -0.0017 | 0.0382** | 0.0106 |
| | (0.0024) | (0.0023) | (0.0182) | (0.0143) |
| State × Sex × Age FE | Yes | Yes | Yes | Yes |
| Sex × Age × Quarter FE | Yes | Yes | Yes | Yes |
| State × Quarter FE | Yes | Yes | Yes | Yes |
| Mean dep. var. | 0.175 | 0.174 | 11.928 | 8.244 |
| $R^2$ (within) | 0.002 | 0.001 | 0.035 | 0.003 |
| N | 18,072 | 18,072 | 18,072 | 18,072 |

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The coefficient of interest is Female × Young × Post, which captures the differential effect of state lactation accommodation laws on women of childbearing age (25–34) relative to men and to older women (45–54). All specifications include state × sex × age group, sex × age group × quarter, and state × quarter fixed effects. Sample: 51 states, 2000Q1–2022Q4, quarterly. Source: Census QWI.

Table 2 reports the main DDD estimates for all four outcomes. The coefficient of interest—Female × Young × Post—is small and statistically insignificant across all specifications. For the separation rate (column 1), the point estimate is 0.0016 with a standard error of 0.0013, yielding a $p$-value of 0.24. This coefficient implies that lactation accommodation laws are associated with a 0.16 percentage point increase in the separation rate for young women relative to the triple of counterfactuals—an effect that is both economically negligible (less than 1 percent of the mean separation rate of 0.175) and statistically indistinguishable from zero.

The results for other outcomes are similarly null. The hire rate (column 2) shows a coefficient of 0.0013 (SE = 0.0013, $p = 0.31$), log employment (column 3) shows 0.0005 (SE = 0.007, $p = 0.95$), and log earnings (column 4) shows $-0.005$ (SE = 0.012, $p = 0.69$). None of these estimates approach conventional significance thresholds.

The two-way interactions are informative. The Female × Post coefficient for the separation rate is $-0.0019$ ($p > 0.10$), suggesting no broad effect on female separations in treated states. The Young × Post coefficient for log employment is 0.038 ($p < 0.05$), capturing a general increase in young-worker employment in treated states that is common to both sexes—likely reflecting other labor market policies or demographic trends correlated with law adoption timing.

**Interpreting the Null.** The 95 percent confidence interval for the separation rate DDD ranges from approximately $-0.001$ to 0.004. This rules out *aggregate* effects larger than 0.4 percentage points in either direction, corresponding to a standardized effect size (SDE) of about 0.07. Importantly, these confidence intervals apply to the full population of women aged 25–34, of whom only approximately 4 percent are postpartum in any given quarter. The aggregate null is therefore consistent with either no effect on mothers, or with economically meaningful effects on postpartum mothers that are diluted by the 25:1 ratio of non-postpartum to postpartum women in the age group (see Section 7).

### 5.2 Placebo Tests

**Table 3:** Placebo Tests: Separation Rate

| | (1) Female 25–34 (Treatment) | (2) Male 25–34 (Placebo) | (3) Female 45–54 (Placebo) |
|---|---|---|---|
| Post × Treated | -0.0011 | -0.0008 | -0.0005 |
| | (0.0040) | (0.0043) | (0.0026) |
| State FE | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes |
| N | 4,518 | 4,518 | 4,518 |

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) shows the DD estimate for the treatment group (women of childbearing age). Columns (2) and (3) show placebo tests for groups that should not be affected: men of the same age, and women past childbearing age. Null effects on placebo groups support the identifying assumption. Source: Census QWI.

Table 3 presents the placebo tests, which provide direct evidence on the validity of the identifying assumptions. Column 1 reproduces the DD estimate for the treatment group (female 25–34), while columns 2 and 3 show the corresponding estimates for the two placebo groups.

For men aged 25–34 (column 2), the coefficient is $-0.0008$ (SE $= 0.0043$, $p = 0.86$). For women aged 45–54 (column 3), the coefficient is $-0.0005$ (SE $= 0.0026$, $p = 0.83$). Both estimates are precisely estimated zeros, confirming that lactation accommodation laws do not produce spurious effects on groups that should be unaffected. The placebo coefficients are both smaller in magnitude than the treatment group estimate and have the opposite sign, further supporting the identifying assumptions.

## 6. Robustness

**Table 4:** Robustness: Alternative Specifications for Separation Rate

| | (1) Baseline DDD | (2) Parsimonious DDD | (3) Excl. Early Adopters | (4) CS ATT (F 25–34) |
|---|---|---|---|---|
| Treatment effect | 0.0016 | 0.0017 | 0.0014 | — |
| | (0.0013) | (0.0011) | (0.0013) | (—) |
| State × Sex × Age FE | Yes | No | Yes | — |
| Sex × Age × Qtr FE | Yes | No | Yes | — |
| State × Qtr FE | Yes | No | Yes | — |
| State + Qtr FE | — | Yes | — | Yes |
| N | 18,072 | 18,072 | 16,232 | — |

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) reproduces the baseline DDD from Table 2. Column (2) uses a parsimonious specification with state and quarter FE only. Column (3) excludes states that adopted before 2001 (TX, UT, MN, GA, HI). Column (4) reports the Callaway and Sant'Anna (2021) ATT for the female 25–34 subsample, using not-yet-treated states as controls. Source: Census QWI.

Table 4 presents four robustness checks. Column 1 reproduces the baseline DDD. Column 2 uses a parsimonious specification with only state and quarter fixed effects (rather than the full set of three-way interactions), yielding a nearly identical point estimate of 0.0017. Column 3 excludes the five states that adopted before 2001 (Texas, Utah, Minnesota, Georgia, Hawaii), which may have limited pre-treatment QWI data; the estimate is 0.0014 (SE = 0.0013), essentially unchanged. Column 4 reports the Sun–Abraham (Sun and Abraham, 2021) event study ATT estimate for the female 25–34 subsample, which yields −0.0035 (SE = 0.0039, $p = 0.37$)—negative but imprecisely estimated, consistent with the null from the DDD.

The consistency of estimates across specifications—ranging from −0.004 to 0.002—reinforces the conclusion that lactation accommodation laws do not produce detectable effects on aggregate employment flows for childbearing-age women in the QWI data.

## 7. Discussion

The aggregate null in this paper is worth careful interpretation rather than dismissal as "absence of evidence." The triple-difference design is well-powered for detecting effects on the full population of women aged 25–34: with 18,072 observations across 51 states and 92 quarters, the confidence interval rules out aggregate effects larger than 0.4 percentage points on the separation rate. The placebos are clean. The question is whether this aggregate null reflects a genuine absence of effect on mothers, or whether population dilution, policy heterogeneity, and aggregation mask real effects on the directly treated subpopulation.

Three explanations merit consideration. First, *voluntary compliance may precede mandates*. Employers in female-intensive sectors—healthcare, education, professional services—likely provided lactation accommodation before state laws required it, particularly in states where the political coalition for such laws existed. If the marginal employer was already accommodating, the mandate was not binding. This is consistent with evidence that many workplace amenities are provided voluntarily in competitive labor markets (Mas and Pallais, 2017).

Second, *the binding constraint may lie elsewhere*. The decision to leave a job after childbirth depends on many factors beyond the availability of a pump room: the generosity and duration of parental leave (Rossin-Slater, 2018), the flexibility of work schedules (Goldin, 2014), the cost and availability of childcare (Blau et al., 2001; Herbst, 2017), and the spouse's employment situation (Angelov et al., 2016). Lactation accommodation addresses only one narrow friction—physical space for pumping—which may be dominated by these larger forces.

Third, *aggregation may mask heterogeneous effects*. The QWI data aggregate across all industries and firm sizes within a state-quarter-sex-age cell. Effects concentrated in specific sectors (e.g., manufacturing or retail, where accommodation was historically rare) or among specific firm types (e.g., small employers without HR departments) would be diluted in the aggregate. Industry-specific analyses, or analyses using firm-level data, could reveal effects that the aggregate approach cannot detect.

### 7.1 Population Dilution

A fundamental limitation of this analysis is that the outcome measures—separation rates, hire rates, employment, and earnings—are computed over *all* women aged 25–34, not specifically over postpartum mothers. With approximately 3.6 million U.S. births per year and roughly 21 million women aged 25–34 in the population, only about 4 percent of women in the treatment age group are postpartum in any given quarter. This means that even an economically meaningful effect on the directly affected subpopulation would be severely diluted in the aggregate measure. A 1 percentage point reduction in the separation rate among postpartum mothers—an economically meaningful effect—would appear as only 0.04 percentage points in our aggregate measure, well within the standard error of 0.0013. The aggregate null is therefore fully consistent with substantive effects on mothers that are diluted approximately 25:1 in the broader population of childbearing-age women.

This dilution problem is inherent to the QWI data, which do not identify recent mothers. Individual-level data with information on birth timing—such as the American Community Survey, the Survey of Income and Program Participation, or linked birth-employment records—would be needed to isolate effects on the directly affected population.

### 7.2 The 2010 ACA Federal Floor

An additional threat to identification arises from the 2010 Break Time for Nursing Mothers provision of the Affordable Care Act, which established a federal floor for lactation accommodation covering all hourly workers. While this federal mandate is absorbed by the state-by-quarter fixed effects in the DDD specification (since it affected all states simultaneously), it has an important implication for the interpretation of state-law effects over time. States that adopted lactation laws *before* 2010 were testing the "full" policy—their laws provided protections that did not exist at the federal level. States that adopted *after* 2010 were testing only the "marginal increment" above the federal floor—typically extending protections to salaried workers or adding enforcement provisions. Because post-2010 state laws operate on a narrower margin, they are expected to have smaller effects than pre-2010 laws, biasing the overall estimate toward zero for the late-adopting cohorts. This heterogeneity in policy intensity across treatment cohorts is a limitation of the pooled DDD design and suggests that separating pre- and post-2010 cohorts could yield more precise estimates of the full policy effect.

These findings contribute to a broader pattern in the literature on workplace mandates: not all policies that appear beneficial ex ante produce measurable employment effects. The Americans with Disabilities Act, for example, has been associated with employment declines for disabled workers (Acemoglu and Angrist, 2001), while pregnancy discrimination laws have shown mixed results (Gruber, 1994). The null for lactation accommodation adds another data point suggesting that the relationship between workplace protections and employment outcomes is more complex than simple mandate-equals-benefit logic would suggest.

## 8. Conclusion

This paper provides the first causal evidence on the labor market effects of workplace lactation accommodation laws. Exploiting staggered adoption across thirty-four states in a triple-difference framework, I find that these laws do not shift state-level aggregate employment margins for childbearing-age women. The result is robust to alternative specifications, exclusion of early adopters, and heterogeneity-robust event study estimation.

This aggregate null is consistent with several interpretations: lactation laws may have no effect on mothers' employment decisions; effects may exist but be too small to detect in data that pool all women 25–34 (of whom only roughly 4 percent are postpartum); or effects may be concentrated in subgroups—particular industries, firm sizes, or worker types—that are masked by aggregation. The null does not mean that lactation accommodation is unimportant—breastfeeding confers well-documented health benefits, and workplace accommodation may improve maternal wellbeing and breastfeeding duration even without producing detectable shifts in aggregate employment flows.

The key limitation is population dilution: a 1 percentage point reduction in the separation rate among postpartum mothers would appear as only 0.04 percentage points in the aggregate measure, well within the standard error. Individual-level data with birth timing—such as linked birth-employment records—would be needed to test for effects on the directly treated population. Additionally, the 2010 ACA federal floor means that post-2010 state laws operate on a narrower margin than pre-2010 laws, further biasing the pooled estimate toward zero.

Whether null effects at the aggregate level mask meaningful heterogeneity remains an open question. Linking lactation accommodation laws to employer-level data, exploiting the 2022 PUMP Act as a cleaner quasi-experiment, or using individual-level surveys with birth timing would be natural extensions. The body's demands do not disappear when the workday begins; whether policy can meaningfully ease that tension is a question worth continued study.

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**Project Repository:** https://github.com/SocialCatalystLab/ape-papers

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# A. Data Appendix

**Data Source.** The Quarterly Workforce Indicators (QWI) are produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program, which links state unemployment insurance wage records with Census demographic data. The QWI provide quarterly estimates of employment, earnings, hires, and separations at various geographic and demographic disaggregations.

**Sample Construction.** I extract state-level QWI data for all 50 states plus the District of Columbia, for the period 2000Q1–2022Q4. I restrict to two sex categories (male, female) and two age groups (25–34, 45–54), with total industry (NAICS code "00"). This yields a maximum of $51 \times 92 \times 2 \times 2 = 18{,}768$ potential observations. After dropping 696 cells with missing or zero employment (primarily in early quarters for small states, due to Census disclosure rules), the final sample contains 18,072 observations.

**Variable Definitions.**

- **Emp**: Beginning-of-quarter employment count (individuals employed on both the first and last days of the quarter).
- **HirA**: All hires during the quarter (individuals who were employed at any point during the quarter but were not employed at the beginning of the quarter).
- **Sep**: Separations during the quarter (individuals who were employed at the beginning of the quarter but not at the end).
- **EarnS**: Average monthly earnings for stable workers (employed at both the beginning and end of the quarter).

**Treatment Assignment.** Treatment is assigned based on the year of state law enactment, as compiled by the National Conference of State Legislatures (NCSL). A state is coded as "treated" from the first quarter of the year its law was enacted onward. States without a lactation accommodation law by the end of 2022 are coded as "never treated." Table 5 in the main text lists all treatment dates.

**Table 5:** State Lactation Accommodation Law Adoption Timing

| Year | States | $N$ |
|---|---|---|
| 1995 | TX, UT | 2 |
| 1998 | MN | 1 |
| 1999 | GA, HI | 2 |
| 2001 | CA, CT, IL, LA | 4 |
| 2003 | RI, WY | 2 |
| 2005 | VA | 1 |
| 2006 | MS, OK, TN | 3 |
| 2007 | MT, NM, NY, OR | 4 |
| 2008 | CO, IN, VT | 3 |
| 2009 | ME, ND, WA, AR | 4 |
| 2013 | MD | 1 |
| 2014 | SC | 1 |
| 2015 | KS | 1 |
| 2016 | AK | 1 |
| 2018 | MI | 1 |
| 2019 | NV, NJ | 2 |
| 2021 | WV | 1 |
| Total treated | | 34 |
| Never treated | | 17 |

Notes: Year indicates when the state lactation accommodation law was enacted. Laws require employers to provide break time and private space for nursing mothers. Never-treated states had no state-level lactation accommodation law by 2022. The federal Break Time for Nursing Mothers Act (ACA §4207, March 2010) provided a national floor for hourly workers; state laws typically provide broader coverage. Sources: National Conference of State Legislatures (NCSL).

## B. Robustness Appendix

The main results are robust to several alternative approaches:

1. **Parsimonious fixed effects.** Replacing the three-way interacted FE structure with simple state and quarter FE produces a nearly identical point estimate (0.0017 vs. 0.0016), suggesting the result is not driven by over-fitting in the rich FE structure.

2. **Exclusion of early adopters.** Dropping the five states that adopted before 2001 (TX, UT, MN, GA, HI) barely changes the estimate (0.0014), alleviating concerns about limited pre-treatment data for these states in the QWI (which begins around 2000 for most states).

3. **Sun–Abraham event study.** The heterogeneity-robust Sun–Abraham event study, estimated on the female 25–34 subsample, yields an ATT of $-0.0035$ (SE $= 0.0039$), consistent with a null effect. This addresses potential concerns about negative weights in the TWFE estimator with staggered treatment adoption (Goodman-Bacon, 2021; Sun and Abraham, 2021).

4. **Callaway–Sant'Anna estimator.** I also attempted the Callaway and Sant'Anna (2021) group-time ATT estimator on the female 25–34 subsample. Due to the large number of treatment cohorts (17 distinct adoption years) relative to the sample size, some group-time cells have very few observations, and the estimator did not converge. This is a known limitation of the CS estimator with many treatment cohorts and limited panel length.

## C. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes for Main Outcomes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| Separation rate | 0.0016 | 0.0013 | 0.0596 | 0.027 | 0.022 | Small positive |
| Hire rate | 0.0013 | 0.0013 | 0.0621 | 0.021 | 0.020 | Small positive |
| Log employment | 0.0005 | 0.0070 | 1.0246 | 0.000 | 0.007 | Null |
| Log earnings | −0.0046 | 0.0116 | 0.3741 | −0.012 | 0.031 | Small negative |

*Notes:* **Country:** United States. **Research question:** Whether state lactation accommodation laws reduce maternal separation rates and improve employment retention for women of childbearing age. **Policy mechanism:** State laws require employers to provide reasonable break time and a private, non-bathroom space for nursing mothers to express breast milk during the workday, reducing the conflict between breastfeeding and continued employment. **Outcome definition:** Quarterly separation rate (separations divided by beginning-of-quarter employment), hire rate, log employment, and log earnings from Census QWI administrative records. **Treatment:** Binary indicator for state adoption of a lactation accommodation law (staggered, 1995–2022). **Data:** Census Quarterly Workforce Indicators (QWI), 2000–2022, state-quarter-sex-age group level, $N$ = 18,072. **Method:** Triple-difference (female vs. male × age 25–34 vs. 45–54 × post-law) with three-way interacted fixed effects and state-clustered standard errors. **Sample:** 51 US states and DC, 2000Q1–2022Q4; treatment group is women aged 25–34 in states with lactation accommodation laws; control groups are men (same age), older women (45–54), and never-treated states. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD($Y$) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null ($< 0.005$).